of equations; for this purpose adding the sexagesimal equations in seconds to the argument of the next inequality, the circumference of which is divided into 16,000,000 parts. The arguments are likewise increased by twice or thrice the annual equation, which, if attended to by Burckhardt, would have removed some of his principal 32 small equations, allowing several of them to be condensed into one table of double entry. As the paper chiefly consists of algebraical formulæ and numerical calculations, it could be only *announced* to the meeting.

On the Determination of the most probable Orbit of a Binary Star from the assemblage of a great number of observed Angles of Position. By Sir J. F. W. Herschel, Bart. *With some Remarks by the President on a Solution of the same Problem by M. Yvon Villarceau.*

In this paper Sir John Herschel refers generally, for the principle of his method, to a paper published by him in the fifth volume of the *Memoirs* of this Society, the paper (we may remark) in which was given an exposition of the principles by which the orbit of a double star was for the first time actually determined. He now states his conviction that the method there expounded is, on the whole, the best that can be employed; and the object of the present paper is, retaining the original principle (namely, of using only the measured angles of position, and rejecting entirely the measures of distance), and retaining the first step of the original method (namely, of smoothing down the irregularities of the angles as measured, by laying them down graphically, the angles for abscissæ and the corresponding times for ordinates, and then drawing a curve by hand through the points so found, and using that curve as the representation of the real relation between the angles and the times, and measuring from it the times corresponding to angles which differ by 5°, or by 10°, or any other convenient difference); retaining the original method thus far, to complete the investigation by a process entirely algebraical and arithmetical.

Supposing the times corresponding to equal intervals of angle to be taken from the curve above mentioned, the next thing required is $\frac{dt}{d\theta}$ for every 5° or every 10°, &c. This is to be found by the following formula, which requires for application only the finite differences of t for the equidistant values of θ,

$$\frac{dt}{d\theta}=\frac{1}{\Delta\theta}\left\{\frac{\Delta t}{1}-\frac{\Delta^2 t}{2}+\frac{\Delta^3 t}{3}-\&c.\right\}$$

The next step is, to infer from this the true apparent distance of the stars, as it ought to be measured by a perfect micrometer or measuring instrument. Now every determination of an orbit of double stars proceeds on the assumption of an attraction between the two components, and this requires the supposition of descrip-

tion of areas proportional to the time, both in the orbit really described and in the projection of the orbit which we see. Hence we must have $\varrho^2 . \frac{d\theta}{dt}$ = constant = 100 (the unit of the radius vector being for the present arbitrary), and therefore $\varrho = \sqrt{100 . \frac{dt}{d\theta}}$ or $= \sqrt{-100 \frac{dt}{d\theta}}$, according as $\frac{d\theta}{dt}$ is positive (that is, θ increasing in the direction $nfsp$) or negative. Sir John adopts for the unit of angles one degree, and for the unit of time one year.

A series of radii vectores being thus found, corresponding to certain values of θ, the next step is to form from these in numbers the corresponding values of the rectangular co-ordinates $x = \varrho . \cos \theta$ $y = \varrho . \sin \theta$. And, assuming that the force of attraction between the two stars follows the law of the inverse square of the distance, and therefore that the curve really described is a curve of the second order, and consequently that the apparent curve is a curve of the second order, we must make these numerical values of x, y, (as $x_1\,y_1$, $x_2\,y_2$, $x_3\,y_3$, &c.) satisfy the equation $o = 1 + \alpha x + \beta y + \gamma x^2 + \delta x y + \varepsilon y^2$, an equation containing 5 unknown constants. As the number of equations will generally exceed 5, it will be proper to combine them by the method of least squares; and the only question is, what is the function of x and y which shall be supposed *à priori* liable to equal error in all? Sir John Herschel tacitly assumes that the function $B = 1 + \alpha x + \beta y + \gamma x^2 + \delta x y + \varepsilon y^2$ is the quantity which with equal weight throughout is to be made as small as possible, or that $\Sigma\,(B^2)$ is to be minimum. The equations given by this consideration are easily formed, and then α, β, γ, δ, ε, can be determined.

From these numbers the numerical values of the more convenient elements of the apparent ellipse may be found, and from them the elements of the real ellipse may be found. The formulæ for all these transformations are given at length by Sir John Herschel, and they are less complicated than might at first have been feared.

Thus far the elements necessary to produce geometrical coincidence of the concluded orbit with the observed orbit are alone determined. The next operation is to determine those elements which relate to the motion in the concluded orbit. For this purpose, angles being taken from the curve based on the graphical projection, and these angles (which relate to the apparent orbit) being converted into angles in the true orbit by the formulæ lately found, and thus exhibiting true anomalies on the true ellipse, the excentric anomalies are found at once by the formula $u - e \sin u$, and the mean anomalies are found. Then every one of these angles gives an equation of the form

$$w_1 = k . t_1 - l,$$

from the assemblage of which the constants k and l can be found by the method of least squares; and then we have all that is required to form the mean anomaly for any other time t, and con-

sequently (as the elements of the ellipse are known) to form the excentric and true anomalies.

The conversion of a place thus computed in the real orbit into one in the apparent orbit, and the comparison of the distance computed on the arbitrary scale with the distance measured with the micrometer, and the inference as to the true value of the units of the arbitrary scale, are steps which require no particular explanation.

Sir John Herschel holds out the hope of following up this exposition with the details of the application of his method to the star γ *Virginis*.

As an Appendix to Sir John Herschel's paper, it is proper to add that papers have been received by Sir John Herschel from M. Yvon Villarceau (namely, a note on the double star ζ *Herculis*, dated 1849, February 1, a note on the double star η *Coronæ*, dated 1849, March 30, and a letter dated 1849, April 1, containing an exposition of M. Yvon Villarceau's methods), which have been communicated more or less completely to the *Académie des Sciences* of France, and which therefore cannot be received in the ordinary way as a communication to this Society. It is, however, the wish, both of Sir John Herschel and of M. Yvon Villarceau, and it appears in every way desirable, that their results should be made known to this Society, both as containing instructive expositions of a very elegant general method and very curious applications of it, and also as bearing upon any questions which may arise as to the similarity or priority of the methods of Sir John Herschel and M. Yvon Villarceau.

Assuming the law of gravitation, and consequently the law of elliptic movement, as applying generally to the relative motion of two stars in a binary system, M. Yvon Villarceau remarks that the projection of this curve upon the spherical sky (or rather upon a plane perpendicular to the visual ray) will be a curve of the second order, whose equation will be,

$$F = a y^2 + b x y + c x^2 + d y + e x + f = 0,$$

the origin of co-ordinates being one star regarded as a fixed centre of attraction of the other. The object of the next process must be, to adopt this general equation to the particular observations from which the orbit is to be deduced: and here it is to be observed that M. Villarceau does not confine himself either to the measured angles of position or to the measured distances, but uses both, for the formation of the numerical values of x and y corresponding to every observation. Having these numerical values of rectangular co-ordinates, and paying no respect (for the present) to the intervals of time between the observations, the following is the method used to accommodate *geometrically* the curve of the second order to the observed co-ordinates:—

The principle assumed is, that the constants a, b, c, &c. shall be so determined that if the resulting curve be drawn, and if from

every observed place a normal (usually a very short line) be drawn to the curve, then the sum of the squares of these normals, each multiplied by its proper weight, shall be a minimum. This principle, it is almost unnecessary to remark, is imperfect, inasmuch as it does not in any way take cognisance of the laws of movement as connected with *time*; but it will frequently be doubtful, in a problem of such difficulty, whether it is not best to neglect a condition, even of the most essential kind, for the sake of making the solution more simple.

Putting $b^2 D^2$ for $\left(\frac{dF}{dx}\right)^2 + \left(\frac{dF}{dy}\right)^2$, and p for the weight of each determination, M. Villarceau arrives thus at the following equations:—

$$\Sigma \,.\, \frac{pF\frac{dF}{da}}{D^2} = 0, \text{ or } \Sigma \,.\, \frac{py^2F}{D^2} = 0$$

$$\Sigma \,.\, \frac{pF\frac{dF}{db}}{D^2} = 0, \text{ or } \Sigma \,.\, \frac{pxyF}{D^2} = 0$$

&c.,

and he shews how, supposing an ellipse roughly drawn by hand, the value of D may be found graphically; and it will then be possible to solve the equations.

The projected ellipse being thus determined, the real ellipse will be found from the consideration that the origin of co-ordinates is the projection of the focus of the real ellipse, while the centre of the observed ellipse is the projection of the centre of the real ellipse. The formation of the corresponding equations is a not difficult problem of analytical geometry. This transformation, however, is not required till all the other operations are completed.

The points determined by observation are not generally found exactly upon the projected ellipse. In order to have points upon the ellipse which shall be the subjects of further investigation, M. Villarceau transfers the observed points to the ellipse by drawing normals to the ellipse, and taking, instead of the point actually determined by observation, the foot of its normal. If x' and y' be the co-ordinates determined from observation, x and y those of the foot of the normal, then

$$x = x' - \frac{\frac{dF}{dx}}{\left(\frac{dF}{dx}\right)^2 + \left(\frac{dF}{dy}\right)^2} \,.\, F(x', y')$$

$$y = y' - \frac{\frac{dF}{dy}}{\left(\frac{dF}{dx}\right)^2 + \left(\frac{dF}{dy}\right)^2} \,.\, F(x', y')$$

with sufficient exactness.

The next point is, to introduce the consideration of time; and this is to be done by making the areas described by the radius vector in the projected ellipse proportional to the time. The areas can be expressed in terms of the corrected co-ordinates and the constants without much difficulty, the whole of these admitting of further correction if necessary. M. Villarceau remarks that if there are four observed places, the solution of the four equations F (x, y) $=0$ will give four of the quantities a, b, c, d, e, in terms of the fifth; that these four observations will give three areas between which there are two equations of proportion; and that thus, besides the determination of the fifth coefficient, we shall have an equation of condition which must be satisfied, or whose failure will prove that our operations or assumptions are in some part erroneous. When there are more than four equations, all can be used in methods analogous to those which are well understood in other investigations, for correcting the result.

We must, however, express our opinion that this part of the operation appears the most obscure, as well as the most delicate and difficult, of the whole.

M. Villarceau remarks that the final determination of elements will in all cases require observations separated by a considerable interval from the rest.

M. Villarceau has lately communicated to the *Académie* another method.

The following are the principal results in the two cases which M. Yvon Villarceau has specially examined:—

In the instance of ζ *Herculis*, the stars are so unequal that there can be no possibility of confusion between the two. It was seen double in 1782, but there is reason to think that it was seen as only one star between 1795 and 1802, and also between 1828 and 1832. M. Struve, expressing himself very doubtful, seemed to suppose that the periodic time might be about 14 years. (See the *Mensuræ Micrometricæ.*) A valuable series of observations, however, having been made at Pulkowa, extending to 1847, the whole of which have been communicated to M. Yvon Villarceau, he has deduced from them an orbit in which the excentricity = sin 27° nearly, and the periodic time is $36\frac{1}{3}$ years. The measure of 1782 and those from 1826 to 1847 appear to be represented with all desirable exactness. (In comparing the computed and observed angles of position, we are glad to see that M. Villarceau has converted their effects into expressions measured by seconds of arc.) The remarks, too, made by M. Struve about the time of the union of the two stars observed by him correspond exactly to the positions given by M. Villarceau's elements. Those of Sir W. Herschel do not correspond. M. Villarceau suggests that, at a time when the small star really was hidden, Sir W. Herschel may have been misled by a false image of the large star; and that, when the image of the star was deformed, he may have estimated the deformation in the wrong direction. He desires, however, specially to submit these conjectures to the judgment of Sir John Herschel; and we

trust that Sir John Herschel will not decline to undertake the honourable task to which he is invited.

M. Villarceau concludes with pointing out that this star presents a remarkable illustration of the amount of uncertainty which may rest upon the determination of double-star elements, when based upon a limited series of observations. If we had only to satisfy the observations extending from 1828 to 1847 (or through more than one-half of a revolution), we might have represented them by systems of elements in which the excentricity varies from 0·44 to 1·63, that is, the orbit might have been an ellipse, a parabola, or a hyperbola.

In the instance of η *Coronæ* there is a difficulty of a totally different kind. The two stars are so very nearly equal in magnitude and similar in colour, that, when observations are interrupted for a long time, it is impossible to say whether that which is adopted as the zero-star before and after the interruption is the same; and it is therefore necessary in some cases to make double computations, on the two suppositions that the first star, or the second star, is that to which the measures are referred in other observations.

From the observations to which they had access, M. Struve, Sir John Herschel, and M. Mädler, concluded that the periodic time of this star was 43 or 44 years. M. Villarceau, however, has had access to the observations made at Pulkowa from 1826 up to 1847, and has treated them in the following manner:—

Of fifteen observations, four were rejected, on account of manifest errors in the distance only. From the remaining eleven, relations were obtained between the elements, which leave them dependent upon an indeterminate quantity which is arbitrary between very wide limits. The observations of Sir John Herschel in 1823, and of M. Struve from 1826 to 1847, may be represented with sufficient accuracy by ellipses in which the periodic time ranges from 38 to 190 years. To fix this indeterminate quantity, we may take Sir W. Herschel's observation of 1781 or that of 1802 (with a slight alteration sanctioned by Sir John Herschel). If we fix the indeterminate quantity by the observation of 1802, M. Villarceau finds that the observation of 1781 is also satisfied, provided that the position of the stars be reversed; that is, provided that it be assumed that the other star has been used as the zero, which is perfectly admissible. Thus is obtained an orbit with a periodic time of 66 years.

But if we reverse the position of the stars in 1802, which is admissible, it is found that the observation of 1781 is satisfied without reversion. The periodic time thus obtained is 43 years.

It is remarkable that in these totally different solutions the excentricity is sensibly the same, namely, 0·47.

In both cases the remaining errors are so small, in comparison with the probable errors, as to leave the two solutions equally entitled to our reception. For the final judgment between them, M. Villarceau refers to some remarks of Sir W. Herschel, unaccompanied by measures. Although there is some doubt in the

interpretation of these, M. Villarceau thinks that upon the whole the solution which gives a period of 66 years is the more probable. He remarks, however, that in four years at the furthest the doubt will be settled. In 1853·677 the angle of position given by the 66-year solution will be 303° 44′, while that given by the 43-year solution will be 356° 30′, leaving a difference upon which there can be no doubt. The distances will be respectively 0″·51 and 0″·77, but between these it might be difficult to pronounce.

On the Practicability and Advantages of obtaining a Sea-rate for a Chronometer. By H. Toynbee, Esq., late Commander of the Ellenborough, East Indiaman.

Having found by experience that lunar observations taken Sun E. and Sun W.* give very different Greenwich Times, Capt. Toynbee has adopted the following method of combining his results in settling the rate and error of his chronometer:—

Having made numerous careful sets of lunar distances, Sun E. and Sun W., whenever an opportunity occurred, Captain Toynbee works out the error and rate of his chronometer as in the following instance. Having found from the means of several sets on several days,

1848. d	Sun E. of Moon. Error Chron. m s	d	Sun W. of Moon. Error Chron. m s
Aug. 24·0	−3 31·4	Sept. 3·4	−6 1·3
Sept. 18·5	−6 52·2		
Loss in 25·5	−3 20·8	Daily rate −0m 7s·88	

Now, bringing up the error found on Sept. 3·4, Sun W., by 15·1 × −7s·88, or 1m 59s·0, we have the following chron. errors:—

d		m s
Sept. 18·5	Sun E., Chron. slow	6 52·2
	Sun W.	8 0·3
	Mean	7 26·3

With this final error for Sept. 18·5 and rate −7s·88, the chronometer being brought up to October 1st, was found to be slow 9m 4s·8; the flash of the signal-gun at Madras shewed it actually slow 9m 20s·3, *i. e.* the error in longitude was only about 4′. It should be remarked that the chronometer had fallen from its stand in a gale of wind on August 20th, and that Madras was made without reference to any other rate or error than that deduced above, *i. e.* from lunar distances observed after the accident.

A more extended series is added to shew the coincidence of the partial results obtained by Capt. Toynbee on his return:—

* There are many reasons why lunar distances from the sun are preferred to those from the planets and stars, and we believe they are more commonly observed.